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REACTANCE AS A RESULT OF REPEATED THREATS
AND AN INTERVENING RESTORATION
OF ATTITUDINAL FREEDOM

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Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor
of Philosophy in the Department of
Psychology in the Graduate School
of Duke University

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ABSTRACT

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This experiment investigated the effect of successive threats to the same attitudinal freedom on reactance arousal. Although the effect of repeated or successive threats to freedom was not considered in the original statement of reactance theory, later research suggests there will be special effects. A series of threats in close succession may arouse greater reactance than an additive model would predict. One possible explanation for such an increase in reactance is that the initial threat makes later threats more salient. Another possibility is that the later threat helps to clarify the individual's perception of the threatener's intention; that is, repeated threats imply that the behavior is directed toward the individual with a goal in mind. A second factor in the situation is whether the threatened individual has an opportunity to restore freedom between threats. Experiments on the prior exercise of freedom imply

that an intervening reassertion of freedom will weaken the effect of subsequent threats by undermining their credibility.

An experimental situation was created in which subjects were introduced to a (fictional) candidate for appointive office. After reading an initial description of the candidate and completing an initial preference rating, subjects received a pair of communications advocating his selection. Then a final rating of preference was made. The first communication always contained a threat to the freedom to reject the candidate. The first variable was the presence or absence of a threat in the second communication (Two vs. One Threat). The second variable was the presence or absence of an opportunity to restore freedom between the first and second communications (Forced Restoration vs. No Restoration), in the form of an essay pointing up reasons against the selection of the candidate. It was predicted that subjects would exhibit greater negative change (in the direction of greater opposition to the candidate) after repeated threats than after a single threat when no intervening opportunity to restore freedom was permitted. No increase in negative change after successive threats was expected when an intervening restoration was allowed. A third variable, whether the second communication was attributed to the original communicator or a different one (Same vs. Different Source), was added to the design to gather support for either the salience or the perceived intent explanation for repeated threat effects. If the initial threat makes later ones more salient, the source

of the later threat should make no difference. If the later threat clarifies the intent behind the first, then only repeated threats from the same source should produce an increase in negative change.

The results of the experiment were not arrayed as expected. The only significant increase in negative change after repeated threats did occur when no restoration came between them. However, the increase appeared when the threats had different sources rather than the same source. This pattern of results supports neither the salience nor the perceived intent explanations. In addition, the intervening restoration had an unanticipated negative effect on change. These effects were considered as partial confirmation of the hypothesis, and it was proposed that threats from different sources aroused more reactance because they were perceived as different threats, while threats from the same source were viewed as a single, extended threat. Alternative explanations were discussed and issues for future research were raised.

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INTRODUCTION

Brehm's (1966) reactance theory is concerned with a person's response to a specific loss of freedom. The theoretical treatment starts with a particular freedom that is eliminated or threatened. Factors that influence the arousal of the motivation to restore freedom are discussed, and the ways in which the motivation will be expressed are enumerated. As Wicklund (1974) noted, there is no special consideration given to what happens when another threat to freedom follows the first in short succession. Furthermore, the theory does not address itself to the issue of what effect intervening behaviors such as restoring one's freedom might have upon reactions to subsequent events that threaten freedom. These questions form the focus of this paper and the experimental investigation reported within it.

A Statement of the Theory

Before turning our attention to multiple threats to freedom, a review of reactance theory is in order. The theory of psychological reactance (Brehm, 1966, 1972) assumes that every individual possesses a set of behaviors in which he believes himself free to engage. If any of

these behavioral freedoms were to be eliminated or threatened with elimination by someone or something, then the individual would experience reactance. The term "reactance" refers to a motivational state of arousal directed toward the reestablishment of the freedom and against any further losses of freedom.

The magnitude of the reactance aroused is determined jointly by three variables. First, the motivation is a direct function of the importance of the freedoms threatened. Second, reactance increases as the proportion of free behaviors threatened increases. Third, the magnitude of the arousal grows along with the degree of threat until the point of outright elimination is reached.

The arousal of reactance produces both subjective and behavioral consequences for the individual. From a subjective standpoint, reactance leads to an increase in the person's desire to engage in the behavior. From a behavioral standpoint, the motivation tends to result in attempts to restore the threatened freedom. When the freedom is irrevocably lost or direct reestablishment would be too costly, the person may instead engage in acts that indirectly imply that the freedom again exists. The major propositions of the theory concerning both the determinants and consequences of reactance have been well supported by experimental research (see Brehm, 1972; Wicklund, 1974).

Simultaneous Threats to Freedom

The early research on reactance was aimed at demonstrating the phenomenon and verifying the propositions of the theory. Since multiple threats to freedom had no particular status within the theory, investigators took little interest in them. The only ways in which multiple threats played a part in the early experiments were as simultaneously occurring components of a broad threat or as inferred threats arising from the circumstances in which a simple threat was delivered.

An example of the manner in which different modes of threatening a single freedom were brought to bear to produce a stronger reactance effect can be found in the Weiner and Brehm (in Brehm, 1966) field experiment. Two kinds of pressure to take a particular option, verbal and monetary pressure, were independently manipulated. As shoppers entered a supermarket, they were given a printed card requesting them to buy a particular brand of bread with money for its purchase attached. Half the shoppers received a low pressure request politely asking them to try the bread. The other half received a high pressure message demanding that the bread be purchased. The amount of money attached to the card was also varied. For some shoppers only the price of the bread was contained on the card, while for others an additional dime above purchase price had been added as a further inducement. Observers inconspicuously noted whether customers selected the induced bread as they exited through the check-out lanes.

The Weiner and Brehm results suggested that increasing either the verbal or the monetary pressure decreased the percentage of female shoppers who complied with the request. Only the difference between the high and low monetary conditions was significant, however, and then only when verbal pressure was high. Apparently both forms of pressure contributed to the overall threat to freedom presented by the card.

Sensenig and Brehm (1968) performed an experiment to illustrate how a single threat to freedom may become a multiple threat by implication. Pairs of female college students were told that they would write five short essays, each on a different current events issue. The experimenter explained that in order to compare the essays it was necessary for both of them to support the same side of the issue. In the high-
implied-threat and control conditions, the requirement was said to extend to all five essays, but in the low-implied-threat condition subjects were asked to support the same side only on the first essay. Both subjects believed that the other had been chosen to decide which side to support when the experimenter placed them in separate rooms. Each then received a note pertaining to the first essay from the decision-making partner. The note simply dictated the side to support in the threat conditions, while in the control condition a preference was diplomatically expressed. In every case the subject was asked to support the same side of the issue she had favored in a premeasure. After completing the first essay, subjects filled out attitudinal dependent measures

and the experiment was terminated.

Sensenig and Brehm found significantly greater boomerang attitude change in the high-implicit-threat condition than in either the low-implicit-threat or control conditions. The situation in which the threat occurred led subjects to infer that similar threats were likely to follow. Expected threats combined with the instigating threat to produce a greater magnitude of reactance.

Although the Weiner and Brehm (in Brehm, 1966) and Sensenig and Brehm (1968) experiments are of considerable importance in demonstrating the theory, they shed no light on what effects should be expected for successive threats. There is little reason to believe that repeated attempts at coercion will combine in an additive fashion as simultaneous pressures do. Moreover, it would be risky to assume that knowledge of a previous imposition would alter a person's perception of a current one in the same manner as implied future threats do. It remained for Heller, Pallak, and Picek (1973) to conduct the first experiment on successive threats and for Wicklund (1974) to uncover and interpret it.

Successive Threats to Freedom

Heller et al. (1973) sought to investigate the interactive effects of an agent's intent to influence and his actual influence attempt on attitude change. When the undergraduate subjects had completed a pretest they were told that they would write five short essays on different topics.

Then the experimenter began to describe some of the possible topics to the subject and his partner. In the high-intent condition the partner, who was actually a confederate, expressed an intent to persuade others about the target issue when it was mentioned, while in the low-intent condition the confederate declared that he was familiar with, but uninterested in the issue. A no-intent control condition in which the confederate said nothing was also included in the design. After the intent manipulation the procedure followed that of Sensenig and Brehm (1968) closely. The subject and partner were informed that they would write on the same side of the issue for all five topics and that one of them would be selected to act as decision-maker. The subject was led to believe that the confederate had been selected and would be allowed to communicate with him through the sending of notes. As in the Sensenig and Brehm experiment, the threat manipulation came in the form of the decision-maker's note before the first essay. In the high-threat condition the note dictated a side of the issue to support, while in the low-threat condition the same preference was expressed but the subject's opinion was politely solicited. In the no-threat control condition there was no note; the subject simply selected a side of the issue to support. The note in both threat conditions always urged subjects to write in support of their pre-tested attitude. Following the threat, subjects completed the attitudinal dependent measures.

The results of the Heller et al. experiment showed that both high-

threat and high-intent to influence led to greater boomerang attitude change. In addition to the main effects for intent and threat, there was an interaction between the variables due to the presence of the greatest change away from the communicator in the high-intent/high-threat condition. The authors contended that when high threat was prefaced by high intent the subject was able to make a correspondent inference (Jones & Davis, 1965). When either of the variables was low, i.e., when the motivation or the behavior was missing, then the interpretation of the episode as a threat to freedom could not be made with as much confidence.

Wicklund (1974) offered a different interpretation of the Heller et al. study. According to the Wicklund account, the experiment involved not one, but two threats. The intent manipulation was itself a threat because it implied that the confederate would avail himself of any opportunity to pressure the subject. Presumably, the subject experienced reactance on the basis of an inference that his freedom to hold his own opinion on the issue was in danger. Later when the threat manipulation directly challenged a similar freedom, the freedom to take one side of the issue, the threats combined in a multiplicative fashion. Wicklund conjectured that the first threat sensitized subjects, making them especially responsive to subsequent threats.

A certain similarity exists between the Wicklund interpretation and the Heller et al. original. Both see the intent manipulation augmenting

the impact of the threat that follows. For Wicklund, however, it makes the threat stand out in stronger relief, while for Heller et al. it adds to and clarifies the meaning of the threat.

What happened to the reactance aroused by the initial threat? In addition to offering the salience or sensitivity account for the interaction of successive threats, Wicklund argued that the reactance generated by the first threat may affect reactions to the second. Data from a pair of personal space experiments (Albert & Dabbs, 1970; Liberman & Wicklund, in Wicklund, 1974) suggest that when a person is unable to restore freedom the preexisting reactance will tend to "spill over" into subsequent reassertions involving different threats. Although there is no direct evidence that subjects in the Heller et al. experiment were unable to restore their attitudinal freedom following the first threat, they certainly were not presented with any special opportunities to do so. Perhaps the multiplicative effect of the two threats was dependent upon the absence of an intervening reestablishment of freedom. Some recent research on prior exercise of freedom supports this line of reasoning.

Prior Exercise of Freedom

The concept of a prior exercise of freedom was advanced by Worchel and Brehm (1970) to account for some unexpected results. The Worchel and Brehm experiment was designed to demonstrate a large effect of threat to freedom on attitude change. College students who

strongly favored or opposed treating the communist party like any other political party in the United States were asked to read a communication supposedly written by a graduate student for consumption by undergraduates. Actually, there were four versions of the communication. Each student received a communication that was either pro or con on the communist party treatment issue and either contained a high or a low threat to freedom. The high-threat speech was sprinkled with statements like "you cannot believe otherwise" and "you have no choice but to believe this." In the low-threat speech the threatening statements were omitted. The dependent measure was a reassessment of opinion on the issue.

Worchel and Brehm conceptualized the importance of the freedom to disagree with the communication in terms of the discrepancy between the person's own attitude and the position being recommended. Therefore, subjects who read the high-threat speech were expected to experience a threat to a more important freedom, and consequently greater reactance, when the speech favored the side of the issue they initially opposed. Surprisingly, the results obtained were just the reverse. The high-threat speech produced boomerang attitude change only when subjects agreed with the position it advocated. When subjects were in disagreement with the position expressed in the speech, they showed a positive change.

Reexamining their experiment, Worchel and Brehm reasoned that the subjects who disagreed with the speech perceived a threat of lesser

magnitude than those who agreed with it. After all, a person who opposed the position advocated in the communication was being told in the high-threat condition that he could not hold opinions that he already held. Since he had previously engaged in the behavior the communicator tried to rule out, the credibility of the attempt was weakened. He could hardly be expected to take the threat as seriously as someone whose initial views conformed to the dictates of the communication; someone who had not engaged in a prior demonstration of his freedom to resist its pressures.

Wicklund and his associates first replicated the results of Worchel and Brehm (Ferris & Wicklund, in Wicklund, 1974) and then set out to test the prior exercise hypothesis directly (Snyder & Wicklund, in press, Exp. 1). The Snyder and Wicklund experiment was organized as an impression formation study with the male and female undergraduate subjects expecting to receive several pieces of information concerning the target persons. The target persons were a pair of candidates for a fictional marijuana study commission. The initial information was arranged so that Mr. Fitz would appear conservative on the issue of marijuana use, and a less desirable choice for the position on the commission than the liberal, Mr. Thomas. Following the initial information, subjects in the prior exercise condition were asked to write an essay favoring the appointment of Fitz, while subjects in the no prior exercise condition wrote on an unrelated topic. Subjects' attitudes toward the two candidates

were premeasured at this point to determine the effect the essays had on opinions and to establish a baseline for change. The next piece of information received by subjects was a persuasion communication arguing the merits of placing Thomas on the commission. In the high-threat condition the communication began with the statement, "There are several reasons why I think Thomas is the only rational choice," and ended with the conclusion, "There is no question that Adrian Thomas is the man for the job." In the low-threat condition these high pressure remarks were omitted, but otherwise the text was the same. A final measure of subjects' attitudes toward the contenders for the commission marked the end of the experiment.

The results of the Snyder and Wicklund experiment strongly supported the notion of a prior exercise of freedom. Although the prior exercise variable had no differential effects on opinion at the time of the premeasure, it had a clear effect on subjects' reactions to the communication. The students who had previously demonstrated their freedom to endorse Fitz through their essays showed no more change away from the communication in the high- than in the low-threat condition. In contrast, students who had written on another topic, and were thus denied an opportunity to demonstrate the freedom, displayed greater negative change when they received the high- than when they received the low-threat communication.

After finding essentially the same pattern of results in a second

experiment using a streamlined version of the above procedure, Snyder and Wicklund (in press, Exp. II) ventured a guess that something like a catharsis effect might happen in the case of repeated reactance arousal and reassertion. Suppose a person experiences several threats to the same freedom in succession. After the first threat the person acts to restore freedom. This act should operate as a prior exercise of freedom in regard to the second threat in the series. Consequently, the ability of the second threat to arouse reactance is weakened and the restoration effect is reduced. With each new threat to freedom in the chain, the amount of prior exercise is growing, while the magnitude of the threat is further eroded. Eventually, the "threat" should have little if any effect on the person, because it is no longer perceived as a threat.

Predicting the Effect of Repeated Threats:
Increments or Reductions in Reactance

The data reviewed in the preceding paragraphs suggest that successive threats have a complex effect on reactance arousal. On the basis of the Heller et al. (1973) results, it seems reasonable to say that successive threats should strongly enhance reactance. On the basis of the Snyder and Wicklund (in press) findings, however, it seems likely that successive threats will mitigate reactance. The crucial difference between the two forecasts is whether or not the individual has the opportunity to reassert his freedom between threats. This leads to the prediction that when subjects are prevented from engaging in a restoration

of freedom following the first of two threats, an increment in reactance should be observed; but when subjects are led to reassert their freedom after the initial threat, a reduction in reactance should be expected.

How can the multiplicative effect of successive threats that occur without an intervening reestablishment of freedom be explained? Is it a simple matter of the initial threat sensitizing the individual to the presence of subsequent threats, or does the first threat alter the meaning of the second? If Wicklund's (1974) salience explanation is correct, then the source of the second threat should be immaterial. The person should be sensitized whether the threats come from the same communicator or not. On the other hand, the first threat may change the person's interpretation of the second by leading him to view both as part of an intended coercive attempt, as alleged by Heller et al. If this is how the process operates, then the perception of intent and an increase in reactance should occur only when both threats arise from the same source. Only a previous action on the part of the same communicator could be of help in clarifying his motives.

In order to test the hypothesis that the presence or absence of an intervening restoration of freedom determines whether successive threats diminish or extend the effects of reactance, an experiment was conducted. Subjects were exposed to either one or two independent threats and either forced to reassert their attitudinal freedom or prevented from doing so after the initial threat. So that the salience explanation for increased

reactance following repeated threats could be pitted against the perceived intent explanation, the experiment also included a manipulation of the source of the second communication.

The principal prediction forecasted an interaction between the number of threatening communications received and the presence or absence of a restoration of freedom following the first threat. Subjects prevented from reasserting their freedom were expected to exhibit more negative attitude change after a pair of successive threats than after only one. Subjects induced to reestablish their freedom were expected to show no additional negative change following a second threat.

Support for either the salience explanation or the perceived intent explanation for the expected enhancement of reactance following repeated threats was dependent upon the effect of the source variable. According to reasoning based upon the salience explanation, the source of the second communication should not interact with the number of threats and restoration variables. The first threat should sensitize the individual, making the second more impactful. Greater negative change should occur no matter who the communicator might be. On the other hand, the perceived intent explanation required the source variable to interact with the other experimental variables. Only a second threat from the same communicator could confirm the motivation underlying the first threat. The perceived intent of the threatener, now clarified for the individual, should lead to increased negative change. The decision between these rival explanations was left to the data.

METHOD

Design Overview

The experimental design was a 2 x 2 x 2 factorial. All the manipulations and dependent measures were given in booklet form. Subjects were led to expect several pieces of information about a particular candidate for the chairmanship of a state committee in connection with a study of impression formation. The first booklet contained a description of the candidate and obtained an initial rating of subjects' preference for the candidate as chairman.

The second booklet distributed by the experimenter contained a communication from a particular source advocating the selection of the candidate and threatening attitudinal freedom by insisting that subjects agree. The first variable was manipulated through written instructions following the communication either to write an essay arguing against the candidate's selection (Forced Restoration) or to write an essay describing other state committees that were needed (No Restoration).

The last booklet passed to subjects by the experimenter manipulated the second and third variables through variations in another communication. This communication was either attributed to the same source as

before (Same Source) or to a different source (Different Source). In addition, the communication either demanded that subjects favor the candidate (Two Threats) or merely advocated his selection (One Threat). Also included in the last booklet was a final rating of subjects' preference for the candidate and additional dependent measures.

Control conditions were added to the design to assess the effects of the initial threat and the Forced and No Restoration manipulations, and to evaluate the impact of the second threat. The No Second Communication conditions, Forced Restoration-No Second Communication and No Restoration-No Second Communication, were included to serve as a baseline measure of reactions to the initial threat and to determine whether the Restoration manipulations produced attitude change before subjects even received the second communication. Such pre-second communication differences might provide alternative explanations for postcommunication effects. To evaluate this possibility, subjects in these controls were asked for a final preference rating of the candidate in the second booklet following the essay.

The Second Threat Control conditions, Second Threat Only-Same Source and Second Threat Only-Different Source, were included to determine whether some aspect of the second threatening communication itself, rather than the coupling of successive threats, might account for differences in attitude change between the One Threat and Two Threats conditions. In the Second Threat Controls, the threatening sentences

were omitted from the initial communication received by subjects in the second booklet. Both Second Threat Controls received the No Restoration essay instructions.

Subjects

Three hundred and forty-three students from the junior and senior classes of Roanoke Rapids High School participated as subjects in the experiment. All subjects were run during a single school day in the time period normally reserved for their English classes. On the day before the experiment, their teachers announced that some graduate students from Duke University would be coming to the classes to conduct an opinion survey. Subjects were run in groups ranging in size from 13 to 37 students. The sessions were approximately 30 minutes in length.

Of the 332 subjects who answered the demographic items, 175 (53%) were juniors and 157 (47%) were seniors. There were 161 (48%) males and 171 (52%) females in the sample. The mean age was 17.24 years.

Data from 23 subjects were deleted from the analyses. Nineteen of these subjects either voiced suspicion or discussed the manipulations with each other during the experiment. (These 19 subjects were distributed across all of the conditions with from one to three subjects appearing in any single condition.) Three subjects were eliminated because they were opposed to the selection of the candidate on the premeasure. One subject was not included due to a procedural error in the distribution

of the booklets. The remaining 320 subjects were assigned to the eight experimental and four control conditions on the basis of a prearranged ordering of the booklets. The booklets were distributed in blocks of 12, one to each condition, to insure approximately equal numbers in all conditions. The order of the 12 booklets within blocks was constant.

Experimenters

Three male graduate students acted as experimenters and a female graduate student assisted in distributing the booklets. Each experimenter ran subjects during five of the six periods during the school day. Whenever the class schedule permitted, experimenters who were not assigned a class of their own assisted in distributing booklets for those who were running subjects. Experimenter 1 ran 90 subjects, Experimenter 2 ran 125 subjects, and Experimenter 3 ran 128 subjects. All three experimenters ran subjects in all conditions whenever they conducted a session.

Procedure

When the subjects had reported to their English classroom and taken their seats, their teacher introduced the experimenter by saying:

This is _____. He is a graduate student at Duke University and he has come to our class today to ask us to take part in an opinion survey. I'll let him explain more about it.

Then the experimenter gave the following instructions to the subjects before each session:

As [the teacher] has told you, today we would like you to participate in a study of how people form impressions and opinions

about others. During the next half hour we will give you several different pieces of information about a particular person, James Tyson. We will present this information to you in different parts and at different points in the study. By doing it this way you will form your impression of Mr. Tyson in pretty much the same way you would in the real world. In your everyday life you form your impressions gradually because you are always finding out more about people.

Please read each set of information carefully when it is passed to you. After each set of information there will be instructions to answer questions or write about your feelings. Go through each form that we give you page by page, following the written instructions as you come to them. It is very important that no one talks to his neighbor during the study. If you are not sure what you are supposed to do, raise your hand and I will come over and answer your question. At the end of the study I will explain more about our research. Once again, please do not talk to each other during the study.

During the course of the study I will give you three different forms. We'll begin now with Form 1. Please read the description and answer the questions it contains. When you have finished, raise your hand and I will give you Form 2. When you have finished with the reading and writing it asks for, raise your hand again and I will give you the last form, Form 3. Are there any questions before we start?

Description

After answering any questions raised by subjects, the experimenter distributed a copy of Form 1 to everyone, each with a different number in the top right corner of the cover sheet. Form 1 contained an account of James Tyson's candidacy for the chairmanship of a state committee and a brief biographical description of the candidate. The description read:

Currently there is a position open in the North Carolina government of Chairman of the Governor's Committee on Jobs for Young People. This committee will have responsibility during the next few months to study the issues concerning the availability and

importance of part-time and summer employment for students and graduates of North Carolina schools. When they have finished their study, the committee will report to the Governor concerning whether the state should attempt to create more jobs for students, or whether the money could be better spent in other areas.

The top candidate for the chairmanship of the Committee on Jobs is James Tyson. Here is a brief description of Mr. Tyson:

James Tyson

Born in Charlotte, N.C. in 1935.

Resident of Rocky Mount.

Graduated from the University of North Carolina as a business and government major. Later earned a master's degree in business administration from U.N.C.

Has operated his own office equipment and supplies business for most of his career.

Belongs to the Methodist Church, the North Carolina Businessmen's Association, and the Junior Chamber of Commerce of Rocky Mount. Spent one year in Washington, D.C. as a board member of the American Council of Small Businesses.

When asked about the need for more part-time and summer jobs for students in North Carolina on previous occasions, Mr. Tyson has expressed a willingness to consider a wide range of proposals and programs. Mr. Tyson also has a good relationship with young people and is often invited to talk to student groups and clubs.

In actuality both James Tyson and the Committee on Jobs were fictional. The procedure and manipulations used in the experiment were adapted from those of Snyder and Wicklund (in press, Exp. II). The issue of who would head a state committee on jobs was chosen with the expectation that it would have considerable importance for our high school student subjects, many of whom would soon be seeking permanent or summer employment. According to Brehm's (1966) theory of reactance, the more important the freedom, the greater the reactance aroused when that freedom is threatened.

The characteristics of James Tyson were deliberately crafted to make him appear as an experienced, knowledgeable liberal who could be expected to understand and sympathize with students' needs for jobs. Tyson was made a resident of a nearby community to increase further his attractiveness for subjects. It was necessary that subjects prefer Tyson initially (or at least not oppose him) for two reasons. First of all, other theories of attitude change, for example, dissonance theory (Festinger, 1957), might predict boomerang effects when subjects oppose the position taken in the communication. Tyson's favorability as a candidate was insurance that such alternative explanations for the expected pattern of results would not arise. Second, the previous experiments that included subjects both pro and con on the attitudinal issue (Ferris & Wicklund, in Wicklund, 1974; Worchel & Brehm, 1970) found little evidence for reactance when subjects opposed the position required by the threat. Therefore, creating a positive initial orientation toward the candidate was viewed as a precondition for maximizing reactance arousal.

Premeasure

Beneath the description were instructions to subjects to indicate "How much are you in favor of, or opposed to, making James Tyson the Chairman of the Committee on Jobs," on an 11-point scale running from -5, labeled "Very much opposed," to +5, labeled "Very much in favor," with the midpoint labeled "Don't care." This item served as the initial measure of preference.

First communication

When subjects finished Form 1 and raised their hands, the experimenter collected the forms and provided them with a copy of Form 2 bearing the same number on its cover sheet as before, assigning them to a prearranged condition. On the first page of Form 2 was a communication advocating the selection of Tyson as committee chairman attributed to a college administrator, Franklin Bradshaw. For subjects in the eight experimental conditions and the two No Second Communication conditions, it read as follows:

Franklin Bradshaw, an administrator at North Carolina State University, has commented on the merits of James Tyson for the post of chairman of the Governor's Committee on Jobs for Young People:

"There are a number of reasons why you must conclude that Tyson is the only rational choice. First of all, his location in Rocky Mount puts him close to a number of students seeking part-time and summer work. Close contact with the people who will need short-term employment in the middle and eastern areas of the state can be important in conducting an adequate study of the situation.

"Secondly, his background in business organizations puts him in touch with the individuals who do the hiring. James Tyson is sensitive to the interests and desires of both management and labor.

"Tyson's earlier statement that he is willing to consider different proposals concerning short-term jobs indicates that he is flexible on the issue.

"There is no question that James Tyson is the man for the job. You have no choice but to agree with me."

Bradshaw, the source of the communication, was also fictional. His occupation of college administrator was chosen to portray him as a credible, but not necessarily expert, communicator in regard to the issue.

The arguments in favor of Tyson used in the communication were based closely on the description subjects had read earlier. This was to avoid the introduction of any new information supporting Tyson's selection, perhaps weakening the threat.

The initial and final statements demanding that subjects agree with the communicator were intended to arouse reactance by threatening their attitudinal freedom to oppose Tyson for the post. The subjects in the two Second Threat Control conditions received copies of the communication in which these demands were omitted. Instead, the communication began with the statement, "There are a number of reasons why I believe that Tyson would be a good choice," and ended with the statement, "James Tyson would be a fine man for the job." These conditions were added to the design, so that the effects of the second communication could be examined separately from those of the first freedom-threatening communication.

Restoration of freedom variable

The second page of Form 2 contained the restoration of freedom manipulation. Subjects received one of the two sets of essay instructions below:

Forced Restoration condition.

We have found that one useful way to gather information about how people form their opinions is to have them argue strongly for one side or the other of an issue. Based upon what you now know, please take a few minutes to write a short statement in the space below

arguing strongly against making James Tyson chairman of the Committee on Jobs for Young People. List some reasons why he might not be a good man for the job.

No Restoration condition.

We have found that one useful way to gather information about how people form their opinions on an issue is to find out how they feel about related issues. In addition to the Committee on Jobs for Young People, it has been suggested the Governor of North Carolina might set up committees to investigate new sources of energy, the prison system, and drug abuse. In the space below, please take a few minutes to briefly describe any committees you would like to see set up by the Governor of North Carolina and explain why.

The page provided sufficient space beneath the instructions to write the requested essay.

The Forced Restoration condition instructions were designed to lead subjects to reassert their threatened freedom to oppose Tyson's selection. Subjects were asked to entertain and defend the very position denied them by the communication. The No Restoration condition instructions, on the other hand, were designed to prevent any restoration of freedom from taking place. Instead, subjects were asked to focus on other topics that had nothing to do with the threatening message.

Subjects in the experimental conditions and the Second Threat Control conditions received no further inserts in Form 2. Subjects in the two No Second Communication conditions, Forced Restoration-No Second Communication and No Restoration-No Second Communication, received an additional page asking them to indicate their preference for Tyson on the same 11-point scale used for the premeasure. This measure was to

be used in conjunction with the premeasure to derive a change score for assessing any direct effects of the restoration manipulation. Also included on the page were three filler items asking subjects how much they favored or opposed having committees set up to study new sources of energy, the prison system, and drug abuse. The items were introduced to reduce suspicion and to justify the essay in the No Second Communication condition. All used the same 11-point scale as the major dependent measure.

Subjects in the Second Threat Control conditions were always given the No Restoration essay instructions. Since these control conditions were included to measure the effects of the second threatening communication when the first contained no threat, the intervening Forced Restoration instructions were inappropriate. There could be no meaningful restoration of a freedom that had not been threatened beforehand.

Source of second communication variable

When subjects raised their hands after completing Form 2, they were given a copy of Form 3 bearing the same number as the Forms 1 and 2. On the first page of Form 3 was another communication advocating selection of Tyson attributed to one of two sources:

Same Source condition.

On another occasion, Mr. Bradshaw of the N.C. State administration had this to say about the value of James Tyson as chairman of the Committee on Jobs for Young People:

Different Source condition.

Jeffrey Rigsbee, a member of the North Carolina House of Representatives, had this to say about the value of James Tyson as chairman of the Committee on Jobs for Young People:

Bradshaw, the communicator in the Same Source condition, was, of course, the source of the earlier communication. Jeffrey Rigsbee, the communicator in the Different Source condition was another fictional individual like Tyson and Bradshaw. His occupation as a member of the State House of Representatives was intended to depict him as a credible, but not necessarily expert, communicator, roughly equal in expertise and status to Bradshaw.

Number of Threats variable

Subjects received one of two versions of the second communication.

In the Two Threats condition the communication read:

You must agree with me that Tyson is the only man for the chairmanship. He has lived in North Carolina all his life; even his college degrees were from a North Carolina university. This background has provided James Tyson with many opportunities to visit with the people of the state and to examine their employment problems.

Furthermore, he has experience in running a business himself. Tyson is acquainted with the hiring practices, both long-term and short-term, that are necessary for a successful business.

Finally, Tyson spent a year dealing with national business problems. He should be familiar with ideas for putting an end to the summer job crunch from across the country.

James Tyson is the only reasonable choice for the position. You cannot disagree with me.

In the One Threat condition, the communication was identical except for the initial and final statements: In this condition, the

communication began with the sentence, "I think Tyson would be a good man for the chairmanship," and ended with the sentence, "James Tyson would be a good choice for the position."

Both communications used the same arguments based upon information in the description of Tyson, but the communication in the Two Threats condition was designed to threaten subjects' freedom to oppose Tyson for the position while the communication in the One Threat condition was not. Thus, subjects in the experimental conditions who received the second threat were exposed to two freedom-threatening communications in the course of the experiment, while subjects who received no second threat were exposed to only one threatening communication.

Dependent measures

The remaining pages of Form 3 following the communication were taken up by the dependent measures. The major measure was a post-manipulation rating of subjects' preference for Tyson as chairman of the committee using the same 11-point scale as the premeasure. This post-measure was to be used in conjunction with the premeasure to derive a change score to serve as the principal measure of reactance arousal.

Following the post-measure of preference were three evaluative items to tap subjects' feelings about the candidate, James Tyson. Subjects were asked how much they liked Tyson and to rate his honesty and

intelligence. These evaluative items were considered to constitute an auxiliary measure of reactance, since the motivation to restore freedom has the subjective effect of decreasing the desirability of an alternative that is forced upon the individual (Hammock & Brehm, 1966).

Next came four items assessing subjects' views of Bradshaw, the source of the first communication. Subjects were asked to judge how qualified he was to comment on the issue, how valuable his comments had been in forming their opinion, how much he wanted to influence them, and how biased he was. Subjects in the Different Source condition were also asked to indicate their views about Rigsbee, the source of the second communication, on the same series of questions.

Three explanatory items were included on the questionnaire as possible indicators of reactance arousal. Subjects were asked how much they cared who was chosen as chairman, how free they felt to decide Tyson was not the man for the job, and how much they felt like disagreeing with the statements made in favor of Tyson. All three items, as well as the items concerning feelings about Tyson and views toward communicators, utilized 11-point scales.

The final items on the questionnaire were a check on which essay the subject had been assigned to write and some demographic questions. The latter included the subjects' job situation, year in school, age, and sex.

When all copies of Form 3 had been taken up, the experimenter

probed the group briefly for suspicion. Then he described the experimental manipulations and their actual purpose in full detail.

RESULTS

As a check-of-sorts on the impact of the Restoration manipulation, subjects were asked to check the topic they had written about earlier in the study. Of the 139 subjects who received Forced Restoration essay instructions, 118 (85%) correctly indicated that they had written "against making James Tyson the Chairman of the Committee on Jobs." Of the 181 subjects who received the No Restoration essay instructions, 160 (90%) responded accurately that they had written "about what other committees the Governor should form." Apparently, the manipulation had considerable impact even in the low-involvement classroom context, since an overwhelming majority of students identified their condition after receiving several forms and measures.

To find out more about what significance the attitude issue involving jobs had for students, subjects were asked to report their current employment status. Of the 312 students who checked one of the options provided, 126 (40%) were looking for a job or would be soon, 161 (52%) had a job, and only 25 (8%) expressed no current interest in a job. In addition, subjects as a group indicated that they cared very much who was chosen to head the committee (overall mean of 7.56 on the 11-point

scale). Taken together, these data support the conclusion that the freedom to favor or oppose the candidate for the chairmanship was sufficiently important to students to allow a fair test of the reactance hypothesis.

Premeasure

When subjects finished the initial description of the candidate, James Tyson, a premeasure of opinion concerning his selection was taken. As can be seen from the initial preference means (see Table 1), the subjects clearly favored Tyson's appointment (overall mean of +3.46). Thus, the preconditions for maximum reactance arousal and elimination of alternative explanations were met.

A cursory examination of the premeasure means in Table 1 uncovers a slight tendency for initial ratings in the Forced Restoration condition to be less positive than those in the No Restoration condition (overall mean of +3.33 vs. +3.60, respectively). An unweighted means analysis of variance (Winer, 1971)¹ was performed on the premeasure ratings to determine if an error in sampling had occurred. The analysis revealed no difference approaching significance for the Restoration variable ($F = 2.29$; $df = 1, 212$; $p > .10$).² Furthermore, all other F -ratios

¹All analyses of variance reported are unweighted means analyses (Winer, 1971) unless otherwise noted. Cell N's vary from analysis to analysis due to cases of missing data.

²All p -levels reported are two-tailed probabilities.

Table 1
Mean Ratings of Initial Preference for Tyson

Number of Threats	Restoration	
	Forced Restoration	No Restoration
One Threat		
Same Source	+3.37 (30) ^a	+3.50 (26)
Different Source	+3.20 (25)	+3.71 (24)
Two Threats		
Same Source	+3.37 (30)	+3.54 (28)
Different Source	+3.37 (30)	+3.67 (27)

Note. -5 = "Very much opposed," 0 = "Don't know," +5 = "Very much in favor."

^aNumber of subjects.

for main effects and interactions were less than one. The distribution procedure for the booklets produced a fairly even assignment of subjects to conditions in regard to extremity on the premeasure.

Repeated Threats and Restoration of Freedom

The experimental hypothesis called for increased negative change in the Two Threats condition where subjects were faced with repeated threats to freedom, but only when there was no intervening Forced Restoration. It was also necessary to establish what, if any, direct effects the Restoration manipulation had on subjects' attitudes toward Tyson's selection. For example, if writing the counterattitudinal Forced Restoration essay somehow bolstered preference for the candidate, then differential attitudes rather than a reestablishment of freedom might explain the expected postcommunication differences. Therefore, an analysis of variance was performed on the attitude change data from the entire 3 (No Second Communication vs. One Threat vs. Two Threats) x 2 (Forced Restoration vs. No Restoration) factorial as a preliminary to the major analysis. The Source variable was not included, since the No Second Communication subjects received no second communication or second source.

Change scores were computed for each subject by subtracting his initial preference for Tyson on the premeasure from his final opinion of the candidate measured after the manipulations. For subjects in the

conditions of the major experimental design, the final measure was taken after the second communication. For No Second Communication subjects the final measure followed the essay. The resultant change score means appear in Table 2.

This preliminary analysis of the change means indicated only the presence of two main effects, a Number of Threats effect ($F = 6.22$; $df = 2, 259$; $p < .005$) and a Restoration effect ($F = 8.07$; $df = 1, 259$; $p < .005$). The Number of Threats main effect was due to more positive change in the One Threat than in either the No Second Communication condition ($F = 4.20$; $df = 1, 259$; $p < .05$) or the Two Threats condition ($F = 5.38$; $df = 1, 259$; $p < .025$). The overall means for the One Threat, No Second Communication, and Two Threats conditions were $+.10$, $-.35$, and $-.29$, respectively. The No Second Communication and Two Threats conditions did not differ from each other ($F < 1$). The initial threat in combination with the essay led to negative change. If the second communication contained no additional threat to freedom (One Threat condition), it produced positive social influence when it arrived. On the other hand, if the second communication contained another threat, there was no positive change. These results are consistent with a reactance theory interpretation, although further negative change was predicted in the event of repeated threats for the No Restoration condition based upon the results of Heller et al. (1973).

The Restoration main effect reflected greater negative change in

Table 2
Mean Change in Preference for Tyson

Number of Threats	Restoration	
	Forced Restoration	No Restoration
No Second Communication	-.64 (22) ^a	-.08 (24)
One Threat (overall)	-.18 (55)	+.40 (50)
Same Source	-.17 (30)	+.35 (26)
Different Source	-.20 (25)	+.46 (24)
Two Threats (overall)	-.42 (59)	-.15 (55)
Same Source	-.63 (30)	+.14 (28)
Different Source	-.21 (29)	-.44 (27)
Second Threat Only (overall)		+.19 (49)
Same Source		+.31 (26)
Different Source		+.13 (23)

Note. -5 = "Very much opposed," 0 = "Don't know," +5 = "Very much in favor."

^aNumber of subjects.

the Forced Restoration condition (overall mean, $-.36$) than in the No Restoration condition (overall mean, $+.08$). However, the difference between Forced Restoration and No Restoration was far from reliable immediately after the Restoration manipulation. The comparison of the No Second Communication condition, Forced Restoration-No Second Communication (mean, $-.64$) and No Restoration-No Second Communication (mean, $-.08$), was not significant ($F = 2.34$; $df = 1, 259$; $p > .10$). Apparently, the Restoration main effect was not entirely produced by some direct effect of writing the essay. This is consistent with the results of Snyder and Wicklund (in press), who found no attitude change after the prior exercise essay.

The effect was chiefly due to differences within the One Threat condition. For One Threat subjects the No Restoration essay led to positive social influence, while the Forced Restoration essay did not ($F = 5.73$; $df = 1, 259$; $p < .025$). The means for the two conditions were $+.40$ and $-.17$, respectively. Within the Two Threats condition, Forced Restoration (mean, $-.42$) and No Restoration (mean, $-.15$) groups again did not differ reliably ($F = 1.35$; $df = 1, 259$; $p > .10$). It would seem that the Restoration manipulation had its greatest effect upon subjects as they received a second communication which did not contain a threat. At this point, those who had written the No Restoration essay were clearly influenced in the direction of the communication. The heightened negativity of the scores in the Forced Restoration condition is quite inconsistent

with the prediction of reduced negative change, or perhaps positive change based upon the Snyder and Wicklund (in press) prior exercise results.

Although the Number of Threats and Restoration variables did not interact ($F < 1$), the only significant difference between the One Threat and Two Threats conditions occurred within the No Restoration condition ($F = 5.15$; $df = 1, 259$; $p < .025$). The One Threat mean within the No Restoration group was $+.40$ and the Two Threat mean $-.15$. This result is in line with the prediction of repeated threat effects only when no intervening reassertion of freedom was permitted. However, the successive threat effect was primarily attributable to positive change in the One Threat condition, rather than the expected additional negative change after Two Threats.

The Salience Explanation vs. the Perceived Intent Explanation

Determination of the process involved in the repeated threat effects expected within the No Restoration condition lay with the Source variable. If repeated threat effects were obtained only in the Same Source condition, the results would tend to support the perceived intent explanation. If, instead, the Source variable had no differential effect on reactions to repeated threats, the salience explanation would receive support. Thus, in statistical terms, a first-order interaction was predicted between the Number of Threats and Restoration variables with the possibility of a

second-order interaction including the Source variable.

Since subjects in the No Second Communication conditions had not received a second communication in the series, these conditions were dropped from the major analysis of the change score means. The major analysis was performed on the resultant 2 (One Threat vs. Two Threats) x 2 (Forced Restoration vs. No Restoration) x 2 (Same Source vs. Different Source) factorial. The condition means appear in Table 2.

The change score means among conditions were not arrayed as expected. When they were subjected to analysis of variance, a main effect for Number of Threats ($F = 5.46$; $df = 1, 211$; $p < .025$), a main effect for Restoration ($F = 6.48$; $df = 1, 211$; $p < .025$), and a borderline Number of Threats x Restoration x Source interaction ($F = 2.94$; $df = 1, 211$; $p < .10$) emerged. As indicated in the preliminary analysis, the Number of Threats effect was caused by more negative change (and less positive change) on the part of subjects who received a pair of threats than those who received a single threat. Apparently, repeated threats led to some additional reactance across experimental conditions.

The Restoration effect was also discussed in connection with the preliminary analysis. As in the earlier analysis, greater negative mean change occurred in the Forced Restoration groups than in the No Restoration groups ($-.44$ vs. $+.11$, overall).

The effects involving the Source variable uncovered in the present analysis were the ones of greatest interest. The weak Number of

Threats x Restoration x Source interaction failed to correspond with its hypothesized form. Rather than stronger repeated threats effects for Same than for Different Source within the No Restoration condition, the observed second-order interaction reflects the opposite ordering among the Source conditions. When no reassertion of freedom was permitted, Two Threats produced significantly greater negative change than One Threat for the Different Source condition ($\underline{F} = 6.62$; $\underline{df} = 1, 211$; $\underline{p} < .025$). The means in the One and Two Threats condition were $+.46$ and $-.44$, respectively. For the Same Source subjects, the Two Threats mean ($+.14$) was slightly more negative than the One Threat mean ($+.35$), but the difference was negligible ($\underline{F} < 1$). Within the Two Threats condition, the Different Source mean was slightly more negative than the Same Source mean, but not significantly so ($\underline{F} = 1.72$; $\underline{df} = 1, 211$; $\underline{p} > .10$).

There were no significant increases in negative change from One to Two Threats under Forced Restoration. In the Different Source condition, the One Threat mean ($-.21$) and Two Threats mean ($-.20$) were the same. The Two Threats mean ($-.63$) was more negative than the One Threat mean ($-.17$) for the Same Source condition, but the difference was far from reliable ($\underline{F} = 2.04$; $\underline{df} = 1, 211$; $\underline{p} > .10$). There was, however, a significant difference between Forced Restoration-Same Source subjects and No Restoration-Same Source subjects within the Two Threats condition on the change measure ($\underline{F} = 5.53$; $\underline{df} = 1, 211$; $\underline{p} < .025$). The means for the two conditions were $-.63$ and $+.14$, respectively. This

difference contributed to the marginal interaction between the three experimental variables.

Despite the unanticipated form of the borderline interaction, some support for the hypothesis can be salvaged. The only significant increase in negative change after successive threats to freedom appeared in conditions where no reestablishment of freedom was allowed. This much of the observed effects is in agreement with the results and conjecture of Snyder and Wicklund (in press). However, the pattern of the interaction offers no support for a resolution in favor of either the salience explanation or the perceived intent explanation of repeated threat effects.³

The Second Threat Controls

The Second Threat Control conditions were designed and appended to the major experimental design to ascertain whether some unusual characteristic of the second threatening communication per se, rather than the two successive threats, might account for Number of Threats differences. This precaution was taken because the communications were not counterbalanced and used different arguments. No threat to

³ Presumably, subjects with more favorable initial preference ratings were less likely to have exercised their freedom to reject Tyson. Consequently, a 2 (Number of Threats) x 2 (Restoration) x 2 (Source) analysis of variance was performed upon the change scores of only those subjects in each condition whose initial ratings were +3 or above to determine if the predicted effects would emerge. The results of this internal analysis of more extreme subjects closely paralleled those of the major analysis, and no new effects or evidence for the hypotheses were obtained.

freedom was included in the initial communication distributed to subjects in these conditions, and the second communication always contained a threat. Both Second Threat Controls, one for Same and one for Different Source, were given the No Restoration essay instructions. Repeated threat effects were expected only in the absence of a restoration of freedom, and, in any case, the Forced Restoration manipulation was inappropriate without a preceding threat.

The change score means for the Second Threat Controls, along with those for the four No Restoration conditions from the major experimental design were subjected to an analysis of variance as a 3 (Two Threats vs. One Threat vs. Second Threat Only) \times 2 (Same Source vs. Different Source) factorial. Although this procedure involved the duplication of some of the comparisons in the major analysis, the new comparisons involving the Second Threat Only conditions were the ones of interest. Means for all six conditions included in the analysis appear in Table 2.

The analysis revealed a Number of Threats main effect ($\underline{F} = 9.90$; $\underline{df} = 2, 148$; $\underline{p} < .001$) and a Number of Threats \times Source interaction ($\underline{F} = 3.68$; $\underline{df} = 2, 148$; $\underline{p} < .05$). The Number of Threats main effect was due to greater negative change in the Two Threats than in the One Threat condition ($\underline{F} = 9.60$; $\underline{df} = 1, 148$; $\underline{p} < .005$) and marginally greater negative change in the Two Threats than in the Second Threat Only condition ($\underline{F} = 3.63$; $\underline{df} = 1, 148$; $\underline{p} < .10$). The One Threat and Second Threat Only

conditions did not differ ($F = 1.32$; $df = 1, 148$; $p > .10$). The overall means for the Two Threats, One Threat, and Second Threat Only conditions were $-.15$, $+.40$, and $+.19$, respectively.

The Number of Threats \times Source interaction mirrored the fact that reliable differences between the Number of Threats conditions occurred only when different sources were responsible for the communications. In the Different Source condition, the comparison between the One and Two Threat means ($+.46$ vs. $-.44$) was significant ($F = 12.48$; $df = 1, 148$; $p < .001$) as was the comparison between the Second Threat Only mean ($+.13$) and the Two Threats mean ($F = 4.89$; $df = 1, 148$; $p < .05$). In the Same Source condition there were no reliable differences between the Number of Threats conditions (F 's < 1) although the Two Threats mean ($+.14$) was more negative than either the One Threat ($+.35$) or the Second Threat Only means ($+.31$).

Apparently, the differences between the One and Two Threats conditions in the major analysis are true successive or repeated threat effects. The Second Threat Only conditions did not produce the amount of negative change the Two Threats condition did. Furthermore, the Second Threat Only conditions were indistinguishable in terms of negative change from the One Threat condition. This potential alternative explanation for the threat effects can safely be ruled out.

Evaluations of the Candidate

Attractiveness rating

Reactance has been shown to increase the desire for a lost alternative and decrease the desire for an alternative forced upon the individual (Hammock & Brehm, 1966). Since the threat manipulation in this experiment involved an attempt to force the candidate upon subjects, a measure of the candidate's attractiveness was expected to reflect reactance arousal. Subjects were asked, "How much do you like James Tyson," and provided with an 11-point scale ranging from -5 to +5 ("Dislike very much" to "Like very much").

The mean attractiveness ratings of Tyson are given in Table 3. The most striking thing about the pattern was its similarity to the pattern observed for the change means in the major analysis (see Table 2). The analysis of variance performed on the attraction means revealed no significant effects, however, despite this similarity. Even so, a simple contrast within the No Restoration-Different condition between the Two Threat and One Threat means (+1.46 vs. +2.46, respectively) yielded a significant threat effect ($F = 4.42$; $df = 1, 212$; $p < .05$). This is precisely the condition in which reliable threat effects on the change measure were found. A decrease in liking after successive threats may reflect reactance arousal.

Table 3

Mean Evaluations of Tyson

Number of Threats	Measure	Forced Restoration		No Restoration	
		Same Source	Different Source	Same Source	Different Source
One Threat	Attractiveness ^a	+1.73 (30)	+1.84 (25)	+2.08 (26)	+2.46 (24)
	Honesty ^b	6.77 (30)	6.44 (25)	6.92 (25)	6.79 (24)
	Intelligence ^c	7.93 (30)	7.64 (25)	7.96 (26)	8.17 (24)
Two Threats	Attractiveness	+1.53 (30)	+1.93 (29)	+2.18 (28)	+1.46 (28)
	Honesty	5.93 (29)	6.38 (29)	6.70 (27)	6.89 (28)
	Intelligence	7.57 (30)	7.00 (29)	7.82 (27)	7.96 (28)

Note. Number of subjects appears in parentheses.

^a -5 = "Dislike very much," 0 = "Neutral," +5 = "Like very much."

^b 0 = "Not at all honest," 10 = "Extremely honest."

^c 0 = "Not at all intelligent," 10 = "Extremely intelligent."

Trait ratings

In addition to the attractiveness rating, subjects were asked to rate Tyson on a pair of personality traits. The trait ratings were intended to provide them with another avenue through which to devalue or reject the candidate. The first of the items was "How honest do you think James Tyson is" ("Not at all honest" to "Extremely honest") and the second was "How intelligent do you think James Tyson is" ("Not at all intelligent" to "Extremely intelligent"). Both employed 11-point scales.

The means for experimental conditions for both the honesty and the intelligence ratings appear in Table 3. Separate analyses of variance performed on each set of means revealed main effects for the Restoration variable. The effect was a borderline one in the case of the honesty ratings ($F = 2.99$; $df = 1, 209$; $p < .10$), but the effect for the intelligence ratings was robust ($F = 4.27$; $df = 1, 211$; $p < .05$). Subjects evaluated the candidate as less intelligent (overall means, 7.76 vs. 7.97) and marginally less honest (overall means, 6.38 vs. 6.83) in the Forced Restoration condition than in the No Restoration condition. These results seem to parallel the Forced Restoration main effect on the change measure (see Table 2). They are but another indicator of the negative influence of the Forced Restoration essay on subjects' views of Tyson and contribute nothing to support the suggestion of a reactance effect for the attractiveness rating.

Evaluations of the Communicators

First communicator ratings

Previous research on reactance has revealed a tendency for subjects to derogate the person responsible for the threat to freedom (e.g., Worchel, 1974; Worchel & Brehm, 1971). Consequently, subjects in the present experiment were asked to make four judgments concerning the source of the first communication, Mr. Bradshaw, and his statements. The questions were: "How qualified is Mr. Bradshaw to comment about who should be chairman for the Committee on Jobs" ("Not at all qualified" to "Extremely qualified"), "How valuable were Mr. Bradshaw's comments in helping you to form an opinion of James Tyson" ("Not at all valuable" to "Extremely valuable"), "How much do you think Mr. Bradshaw wants to influence you about James Tyson" ("Does not want to influence me" to "Wants very much to influence me"), and "How biased is Mr. Bradshaw" ("Not at all biased" to "Extremely biased"). Eleven-point scales followed each question. The conditional means for all four measures are found in Table 4.

The analysis of variance conducted on subjects' mean ratings of how qualified Bradshaw was to comment yielded a Restoration main effect ($F = 4.51$; $df = 1, 209$; $p < .05$) and an interaction between the Number of Threats, Restoration, and Source variables ($F = 6.94$; $df = 1, 209$; $p < .01$). The main effect reflected the fact that, in the Forced Restoration condition (overall mean, 6.15), subjects viewed Bradshaw as less

Table 4

Mean Evaluations of the First Communicator

Number of Threats	Measure	Forced Restoration		No Restoration	
		Same Source	Different Source	Same Source	Different Source
One Threat	Qualified ^a	6.76 (29)	6.24 (25)	6.23 (26)	7.21 (24)
	Valuable ^b	7.27 (30)	5.76 (25)	6.04 (26)	7.00 (24)
	Intent to Influence ^c	8.30 (30)	8.40 (25)	9.32 (25)	8.42 (24)
	Bias ^d	5.35 (29)	6.13 (23)	6.72 (25)	6.38 (24)
Two Threats	Qualified	5.33 (30)	6.31 (29)	6.96 (27)	6.56 (27)
	Valuable	5.67 (30)	6.47 (30)	6.93 (27)	6.26 (27)
	Intent to Influence	9.04 (28)	8.55 (29)	9.37 (27)	9.07 (27)
	Bias	7.41 (27)	7.40 (30)	7.00 (27)	5.93 (27)

Note. Number of subjects appears in parentheses.

^a0 = "Not at all qualified," 10 = "Extremely qualified."

^b0 = "Not at all valuable," 10 = "Extremely valuable."

^c0 = "Does not want to influence me," 10 = "Wants very much to influence me."

^d0 = "Not at all biased," 10 = "Extremely biased."

qualified than in the No Restoration condition (overall mean, 6.73). The interaction was due in part to the appearance of the only One Threat vs. Two Threats difference (means, 6.76 vs. 5.33, respectively) in the Forced Restoration-Same Source condition ($\underline{F} = 7.49$; $\underline{df} = 1, 209$; $\underline{p} < .01$).

As in the previous analysis of the qualified rating, the mean ratings of how valuable subjects considered Bradshaw's remarks revealed a Number of Threats x Restoration x Source interaction ($\underline{F} = 7.14$; $\underline{df} = 1, 211$; $\underline{p} < .01$). Again, the interaction was produced in part by a One Threat vs. Two Threats difference (means, 7.27 vs. 5.67, respectively) in the Forced Restoration-Same Source condition ($\underline{F} = 5.20$; $\underline{df} = 1, 211$; $\underline{p} < .025$).

The analysis of variance of subjects' mean ratings of the degree to which Bradshaw wanted to influence them uncovered at least a marginal main effect for each of the experimental variables: Number of Threats ($\underline{F} = 3.01$; $\underline{df} = 1, 207$; $\underline{p} < .10$), Restoration ($\underline{F} = 4.25$; $\underline{df} = 1, 207$; $\underline{p} < .05$), and Source ($\underline{F} = 3.01$; $\underline{df} = 1, 207$; $\underline{p} < .10$). The most robust was the Restoration effect. Subjects in the Forced Restoration condition perceived less intent to influence on the part of Bradshaw than subjects in the No Restoration condition (overall means, 8.33 and 9.06, respectively). The borderline Number of Threats effect was produced by greater perceived intent to influence in the Two Threats (overall mean, 9.00) than in the One Threat condition (overall mean, 8.60). In addition,

Same Source subjects perceived marginally greater intent to influence (overall mean, 8.98) than did Different Source subjects (overall mean, 8.62).

The analysis of variance performed on subjects' mean ratings of Bradshaw's bias yielded a Number of Threats main effect ($F = 6.04$; $df = 1, 204$; $p < .025$), a Number of Threats \times Restoration interaction ($F = 7.41$; $df = 1, 204$; $p < .01$), and a marginal Restoration \times Source interaction ($F = 2.88$; $df = 1, 204$; $p < .10$). As in the case of the judgments of the intent to influence, the Number of Threats effect was due to greater bias ratings in the Two Threats condition (overall mean 6.95) than in the One Threat condition (overall mean, 6.11). The main effect was qualified by the interaction between Number of Threats and Restoration, however. Only within the Forced Restoration condition was the One Threat vs. Two Threats difference (overall means, 5.69 vs. 7.49, respectively) significant ($F = 14.62$; $df = 1, 204$; $p < .001$). In fact, within the No Restoration condition the means for One Threat (overall, 6.55) and Two Threats (overall, 6.46) were slightly reversed. The marginal Restoration \times Source interaction was caused by a very weak crossover. In the Forced Restoration condition, Bradshaw was rated more biased in the Same (overall mean, 6.34) than in the Different Source condition (overall mean, 6.85), while in the No Restoration condition Bradshaw was seen as more biased in the Different (overall mean, 6.14) than in the Same Source condition (overall mean, 6.87). Neither difference by

itself approached significance (p 's $> .10$).

To summarize the results from the first communicator evaluations, there is some evidence that is consistent with previous studies in which subjects devalued the threatener. In the Two Threats condition, subjects in the present experiment attributed the first communicator with a slightly greater intent to influence them and greater bias than in the One Threat condition. Seemingly, the repeated threats placed more pressure upon the individuals who received them.

Curiously, however, most of the derogation of the first communicator following successive threats occurred in the Forced Restoration condition. Unexpected interactions including the Restoration variable were observed for the qualified rating, the valuable rating, and the bias rating. In every case, the threat effects were quite different than those on the change measure. The change measure results and the predictions imply that strong threat effects for communicator evaluation should appear in the No Restoration condition. This discrepancy will be reconsidered in the Discussion.

There were no consistent effects for the Restoration variable alone on evaluations of the first communicator. Subjects in the Forced Restoration condition judged Bradshaw less qualified than subjects in the No Restoration condition. On the other hand, No Restoration subjects saw a greater intent to influence on Bradshaw's part than Forced Restoration subjects.

First communicator ratings vs.
second communicator ratings

In addition to their evaluations of the first communicator, Bradshaw, subjects in the Different Source condition also evaluated Rigsbee, the second communicator, on the same four questions. This aspect of the procedure allowed a direct comparison to be made between first and second communicator evaluations for Different Source subjects. Presumably, subjects would prefer the second communicator in the One Threat condition, since he would not be responsible for threatening their attitudinal freedom. A special 2 (Number of Threats) \times 2 (Restoration) \times 2 (Communicator) analysis of variance was performed upon the Different Source subjects' ratings of the two communicators on each of the four questions. The Communicator variable was treated as a repeated-measures factor in Winer's (1971, p. 599) unweighted means solution. The conditional means for all four measures appear in Table 5.

Although the ratings for the first communicator were slightly more negative than those for the second (overall means, 6.55 vs. 6.83, respectively) concerning how qualified each was to comment, the analysis of variance revealed no reliable difference ($F = 2.67$; $df = 1, 100$; $p < .10$). The analysis of variance performed on the mean ratings of how valuable Bradshaw's and Rigsbee's comments were was more fruitful, and revealed a pair of marginal first-order interactions. The Number of Threats and Restoration variables interacted ($F = 2.98$; $df = 1, 102$; $p < .10$)

Table 5

Different Source Subjects' Mean Evaluations of the First and Second Communicator

Number of Threats	Measure	Forced Restoration		No Restoration	
		First Communicator	Second Communicator	First Communicator	Second Communicator
One Threat	Qualified ^a	6.24 (25)	6.84 (25)	7.21 (24)	7.33 (24)
	Valuable ^b	5.76 (25)	6.08 (25)	7.00 (24)	7.67 (24)
	Intent to Influence ^c	8.40 (25)	7.72 (25)	8.42 (24)	8.54 (24)
	Bias ^d	6.13 (23)	5.57 (23)	6.38 (24)	5.88 (24)
Two Threats	Qualified	6.31 (29)	6.66 (29)	6.50 (26)	6.54 (26)
	Valuable	6.47 (30)	6.47 (30)	6.26 (27)	6.07 (27)
	Intent to Influence	8.55 (29)	8.55 (29)	8.93 (28)	9.00 (28)
	Bias	7.40 (30)	7.10 (30)	6.07 (28)	6.39 (28)

Note. Number of subjects appears in parentheses.

^a0 = "Not at all qualified," 10 = "Extremely qualified."

^b0 = "Not at all valuable," 10 = "Extremely valuable."

^c0 = "Does not want to influence me," 10 = "Wants very much to influence me."

^d0 = "Not at all biased," 10 = "Extremely biased."

as did the Number of Threats and Communicator variables ($\underline{F} = 2.79$; $\underline{df} = 1, 102$; $\underline{p} < .10$).

The weak Number of Threats \times Restoration interaction was due to a difference between the reactions of subjects in the Forced Restoration condition and those in the No Restoration condition to the absence of a second threat. In the No Restoration condition, the communicators' comments were considered more valuable when the second communication did not contain a threat (One Threat). The mean valuable rating was 6.13 overall in the One Threat condition, and the difference approached significance ($\underline{F} = 3.17$; $\underline{df} = 1, 102$; $\underline{p} < .10$). In the Forced Restoration condition the relationship between the Two and One Threat means (6.47 vs. 5.92 overall) was slightly reversed ($\underline{F} < 1$).

The Number of Threats \times Communicator interaction was caused by a marginal, but anticipated, tendency for subjects to value more highly the comments of the communicator who had not threatened their freedom. In the One Threat condition, where Rigsbee's communication contained no threat, his comments were judged more valuable than those of Bradshaw (overall means, 6.86 vs. 6.37, respectively). This difference was almost significant ($\underline{F} = 3.86$; $\underline{df} = 1, 102$; $\underline{p} < .10$). In the Two Threats condition the relationship between the mean ratings of Rigsbee and Bradshaw (overall, 6.28 vs. 6.37, respectively) was slightly reversed ($\underline{F} < 1$).

When the Different Source subjects' mean judgments of the two

communicators' intent to influence were subjected to an analysis of variance, only a marginal main effect for the Number of Threats variable ($F = 2.90$; $df = 1, 102$; $p < .10$) was uncovered. Subjects attributed slightly more intent to influence to both communicators in the Two Threat condition (overall mean, 8.75) than in the One Threat condition (overall mean, 8.27).

The analysis of variance conducted for subjects' mean ratings of communicator bias in the Different Source condition revealed a pair of borderline main effects for the Number of Threats variable ($F = 3.38$; $df = 1, 101$; $p < .10$) and for the Communicator variable ($F = 3.08$; $df = 1, 101$; $p < .10$), and a weak Number of Threats \times Communicator interaction ($F = 3.33$; $df = 1, 101$; $p < .10$). The Number of Threats main effect for the bias rating was similar to that observed for the judgments of intent to influence. In the Two Threats condition the mean bias rating for both communicators was somewhat higher than in the One Threat condition (overall means, 6.76 vs. 5.99, respectively).

The Communicator main effect was due to greater perceptions of bias on the part of Bradshaw, the first communicator (overall mean, 6.53), than on the part of Rigsbee (overall mean, 6.30). This effect was qualified by the Number of Threats \times Communicator interaction, similar in form to the one observed for the valuable rating. As anticipated, the Communicator effects were entirely attributable to differences within the One Threat condition. In the One Threat condition, the second

communicator, who had not threatened attitudinal freedom, received a lower mean bias rating (overall, 5.72) than the first communicator (overall, 6.26). This difference was highly significant ($\bar{F} = 6.65$; $df = 1, 101$; $p < .025$). There were no differences between communicator bias ratings in the Two Threats condition (both overall means, 6.76).

An inspection of the results from all the analyses comparing the first and second communicators indicates that only borderline effects were obtained. Both communicators were seen as somewhat more intent upon influencing their audience and more biased when subjects received successive threats. Also, within the No Restoration condition the communicators' comments were judged less valuable when threats were repeated. These results are similar to those from the first communicator evaluations for the entire design and, indeed, are based in part upon a portion of those data.

As expected, there was also some tendency for subjects to credit the second communicator when his remarks were not threatening to freedom. In the One Threat condition, Rigsbee's comments were judged more valuable and he was perceived as less biased than Bradshaw, whose name was always linked to the initial threat. Although the effects are weak, they imply that subjects perceived the threats and their sources accurately.

Exploratory Reactance Measures

Three additional items were included on the final questionnaire in the hope that they might serve as indicators of reactance arousal. The questions were "How much do you care about who is chosen as Chairman of the Committee on Jobs" ("Not at all" to "Very much"), "How free did you feel to decide that James Tyson was not the man for chairman" ("Not at all free" to "Completely free"), and "How much did you feel like disagreeing with the statements that were made about James Tyson's good points" ("Not at all" to "Very much"). Each was followed by an 11-point scale. Means for the three items are displayed in Table 6.

Subjects were first asked how much they cared who became chairman as a measure of the importance they attached to the issue. The selection of an important freedom is, of course, a precondition for the arousal of reactance; but on the dependent variable side importance does not afford the clearest avenue to motivational relief. According to Brehm (1966), reactance is not a tension that is reduced in whatever manner possible, but a motivational state with the particular goal of recovering freedom. Hence, a reduction of importance in response to a threat to freedom should be unlikely and "the only reasonable expectation about the effect of reactance on the importance of a lost free behavior is that importance may increase" (Brehm, 1966, p. 11). The analysis of variance performed on subjects' mean ratings on the care item yielded no effects even approaching significance. Brehm's (1966) viewpoint

Table 6

Mean Responses to the Exploratory Reactance Indicators

Number of Threats	Measure	Forced Restoration		No Restoration	
		Same Source	Different Source	Same Source	Different Source
One Threat	Importance ^a	7.77 (30)	7.08 (25)	8.20 (25)	7.83 (24)
	Perceived Freedom ^b	5.63 (30)	4.38 (24)	5.88 (24)	4.46 (24)
	Urge to Disagree ^c	3.97 (30)	2.63 (24)	3.24 (25)	2.21 (24)
Two Threats	Importance	6.96 (28)	7.83 (30)	7.59 (27)	7.25 (28)
	Perceived Freedom	4.82 (28)	4.50 (30)	6.30 (27)	5.71 (28)
	Urge to Disagree	4.07 (28)	3.48 (29)	3.25 (28)	2.46 (28)

Note. Number of subjects appears in parentheses.

^a0 = "(Care) Not at all, " 10 = "(Care) Very much. "

^b0 = "Not at all free, " 10 = "Completely free. "

^c0 = "Not at all, " 10 = "Very much. "

regarding importance as an indicator of reactance has certainly been vindicated.

The question that solicited subjects' perceived freedom to adopt the threatened position encounters the same difficulties from the standpoint of prediction as the importance measure. On the one hand, perceptions of freedom may reflect the limits imposed by the threats. Then, greater reactance should be accompanied by reductions in the perception of freedom. On the other hand, perceived freedom might itself become a mode for expressing reactance. In this event, greater reactance might lead to denial of the threat, as subjects restore freedom by exaggerating how little the pressure affected their behavior.

In this experiment, subjects apparently used the perceived freedom measure to report accurately the pressure placed upon them by the threatening communications. The analysis of variance on the mean ratings of how free subjects felt to decide against Tyson revealed a marginal main effect for Source ($F = 3.43$; $df = 1, 207$; $p < .10$). Perceived freedom was somewhat higher in the Same Source (overall mean, 5.64) than in the Different Source condition (overall mean, 4.78). It will be recalled that the only significant Number of Threats effect for the attitude change measure also occurred in a Different Source condition: No Restoration-Different Source. It may be that the decline in perceived freedom in the Different Source conditions reflects the experience of a stronger threat to freedom from a pair of communicators than from a

single communicator.

The last of the three exploratory reactance measures inquired whether subjects experienced an urge to disagree with the communications. This item was intended to tap directly the motivation to restore freedom, and so bypass any reluctance upon the part of the individual to engage in behavior that would require an alteration or abandonment of his position on the premeasure (see Kiesler, 1971).

When subjects' mean reports of the urge to disagree were subjected to an analysis of variance, two main effects emerged: a marginal effect for Restoration ($F = 3.50$; $df = 1, 208$; $p < .10$) and a highly significant effect for Source ($F = 5.56$; $df = 1, 208$; $p < .025$). The reliable Source effect was produced by reports of a stronger desire to disagree with the communications in the Same Source (overall mean, 3.65) than in the Different Source condition (overall mean, 2.72). The Restoration effect reflected a borderline tendency for Forced Restoration subjects to express a stronger desire to disagree than No Restoration subjects (overall means, 3.58 vs. 2.80, respectively). Unhappily, neither main effect corresponds with the experimental predictions or the observed interaction on the attitude change measure.

None of the experimental reactance measures proved to be a source of support for the predictions. Only the results from the perceived freedom measure are at all consistent with the actual threat effects on the primary reactance measure, attitude change. Particularly disturbing

are the results for the urge to disagree measure. Insofar as this item succeeds in directly tapping the motivation to restore freedom, it casts some doubt on the efficacy of regarding the attitude change results as partial confirmation of the experimental hypotheses.⁴

⁴Correlation coefficients were calculated between several possible pairings of the major and secondary dependent measures. They were uniformly low in magnitude and uninformative and, so, are not reported here.

DISCUSSION

The major results of the experiment may be summarized in the following terms: (a) Greater negative attitude change and more negative evaluations of the first communicator were observed in the Two Threats than in the One Threat condition. (b) The differences in attitude change between the levels of the Number of Threats variable were strongest for No Restoration subjects who received a second communication from a Different Source. (c) The increased negativity in the first communicator evaluations for the Two Threats condition was primarily due to the ratings of all Forced Restoration subjects, in the case of the bias rating, and, more specifically, to the Forced Restoration-Same Source subjects, in the case of the qualified and valuable ratings. (d) In addition to the Number of Threats differences, in the Forced Restoration condition both the candidate (on the attitude change, honesty, and intelligence dimensions) and his advocate, the first communicator (on the valuable rating), received generally less favorable ratings and subjects expressed a somewhat stronger urge to disagree with the communications.

How can such a complex pattern of results be explained? Caution must be exercised in that these findings were in large part unanticipated

and weak in magnitude.

The most promising point of departure in accounting for these data lies with the original hypotheses. The predictions called for successive threat effects, i.e., increased change away from the communication, only in the absence of an intervening reestablishment of freedom. There was also an attempt to identify a boost in negative attitude change following repeated threats as reactance produced by increased salience of the threat or by increased certainty in interpreting the threat as an intentional act. Greater reactance when the same speaker issued both threats would tend to support the perceived intent explanation. Greater reactance when the threats emanate from different sources, as well as from the same communicator, would better fit the salience explanation.

In actuality, the obtained repeated threats effect was not consistent with either prospective explanation. Most of the negative change in preference for the candidate following a pair of threats occurred in a Different Source condition, rather than a Same Source condition. The Source variable made a difference, but not along lines suggested by either the salience or the perceived intent explanations.

Even so, the obtained repeated threats effect offered some encouragement for the hypothesis. The only significant difference between One and Two Threats conditions on the change measure appeared when no intervening reestablishment of freedom was permitted (No Restoration-Different Source). This is consistent with the major

prediction that an intervening restoration weakens the effect of repeated threats by undermining their credibility. At least the successive threats were most effective in a No Restoration condition. Also in line with the prediction were the inflated perceptions of intent to influence on the part of the first communicator registered by No Restoration subjects.

If repeated threat effects within a Different Source condition suggest neither increased salience nor clarified intent, what psychological process is involved? The reactions of subjects in regard to the perceived freedom measure hint at an answer. Different Source subjects felt they had somewhat less freedom to oppose Tyson than did Same Source subjects. Perhaps, when communications come from different quarters, they are more likely to be viewed as multiple threats. When both involve the urgings of a single individual, they may seem to form a single threat. It may be that two communications from two people feel like two threats, but the same communications attributed to a single individual are perceived as parts of a single threat extended in time.

This notion of a perceived isomorphism between communicators and experienced threats might also account for the curious absence of Source effects on three of the four first communicator evaluations. Only in the case of the intent to influence measure did subjects attach a more unfavorable rating to Bradshaw when he was the solitary source for both sets of statements. Surprisingly, subjects seem to have felt no more pressured, for the most part, when his remarks came in two installments

than when they came in one.

The thrust of this explanation can be applied to the Heller et al. (1973) experiment to account for the differences between the results of that study and the present research. In demonstrating the successive threats effect with a single source (threatener), Heller et al. employed altogether different behaviors in constructing their first (intent) and second (threat) threats to freedom. The first threat was an offhand remark, while the second was a written message; and, although both were aimed at attitudinal freedom, it is likely that different freedoms (the freedom not to be influenced vs. the freedom to adopt a particular position) were at stake. In contrast, the present research design operationalized repeated threats as successive instances of the same threat, a pair of written messages raising similar arguments and threatening the same freedom.

Viewed from this perspective, the Heller et al. (1973) results are not at variance with those reported here concerning the effects of single and multiple sources. The issue of whether repeated threats owe their effect to increased salience or to increased certainty about the underlying motivation may be of significance only in situations involving similar, but different, threats. In situations involving nearly identical threats, the experience of successive threats may simply be linked to the proliferation of threateners. Resolution of this issue must await research in which the similarity of repeated threats or the freedoms

they threaten are varied.

There is still the question of the Forced Restoration main effects. Why did subjects in the Forced Restoration condition come to evaluate the candidate and the first communicator more harshly? Perhaps an understanding of these effects can be gained from a closer look at the Forced Restoration manipulation itself.

The Forced Restoration manipulation was intended to reestablish subjects' freedom to reject Tyson by actively engaging them in generating arguments against his selection. Since this activity was a counter-attitudinal one, the subjects having only been previously exposed to positive presentations of the candidate, it is probable that few had attempted to pinpoint the man's weaknesses before. In addition to enabling subjects to reassert their freedom, then, the manipulation may have had the unintended effect of generating further information about Tyson. Subjects became more critical of the candidate because their own counterarguments convinced them. In turn, their lowered ratings of the value of the first communicator's comments may reflect a new awareness of how one-sided and uncritical his remarks were.

Although it is unclear how this more negative attitude toward Tyson could explain away the attitude change data as mere artifact, its presence suggests that something besides a weakening of threat credibility may be involved in Restoration effects. Insofar as subjects adopted a more negative view toward Tyson's selection in the Forced

Restoration condition than in the No Restoration condition, these groups become very similar to Worchel and Brehm's (1970) pro and con subjects. The Worchel and Brehm results imply that the Forced Restoration subjects, being more con on the issue than their No Restoration counterparts, should be less affected by the threat (second threat). However, since the final preference ratings in the Forced Restoration were still decidedly positive (overall mean, +3.05), it does not seem likely that the Restoration effects are primarily due to manipulated differences in attitude. In the Worchel and Brehm experiment, the pro and con groups were extremely polarized on the attitude issue, not just more and less positive.

The preceding attempt to account for the results is admittedly post hoc. Alternative explanations can be offered in its place. For example, it might be argued that the Restoration main effects represent increased reactance. The lowered evaluations of the candidate and negative change in preference in the Forced Restoration condition may indicate additional reactance arousal. Provocative support for this conjecture exists in the form of a slightly inflated urge to disagree with the communications on the part of Forced Restoration subjects.

But, if these effects mirror reactance, the Number of Threats variable should theoretically produce differences. Only in the case of the first communicator ratings were there significant One vs. Two Threats differences for Forced Restoration subjects. Such differences

are missing for the key reactance measure, attitude change. It may be that the reassertion of freedom led subjects to view any communication advocating the candidate as a threat, short-circuiting the effects of the Number of Threats variable. Still, the absence of One vs. Two Threats differences on the principal theoretical indicator of reactance weakens the argument that the intervening restoration of freedom heightened the motivation.

Another interpretation that might be imposed upon the data takes as its thesis that the effects within both levels of the Restoration variable represent reactance. Rather than arguing that either the No Restoration condition or the Forced Restoration condition produced greater increases in reactance after repeated threats, this viewpoint asserts that the Restoration and Source variables altered the mode of reactance arousal without changing the magnitude. Focusing on the One vs. Two Threats differences, for example, perhaps the No Restoration-Different Source subjects expressed reactance via attitude change, while the Forced Restoration subjects channeled the motivation to restore freedom into their evaluations of the threatener (first communicator).

This differential-modes-of-expression result was predicted and apparently obtained by Pallak and Heller (1971). However, the interpretation encounters the same difficulties in regard to the present experiment as it did in the Pallak and Heller study. What considerations were introduced by the experimental variables that would explain the

different forms of reactance expression? Without a compelling answer to this question, the interpretation must be regarded as a rather weak contender.

Although the explanation offered above based upon the original hypotheses seems tenable, the data raise as many questions concerning the role of intervening reestablishment of freedom in successive threat situations as they answer. Repeated threats appear to increase reactance over and above single threats when a reassertion is prevented. On the other hand, the results suggest, if anything, that behavioral engineers and advertising executives would be unwise to plan their programs and campaigns around a series of restrictive communications insisting upon compliance, while providing opportunities for protest or "blowing off steam" sandwiched between. Even if the person at whom these messages are aimed is given a chance to regain his attitudinal freedom, and consequently experiences less reactance as the series of attempts progresses, there is no assurance that he will eventually swing toward a positive view of the message. In fact, the initial opportunities to reassert freedom may leave him with just as negative a view as that of a person prevented from regaining freedom who boomerangs after each communication!

At least on those occasions in which the audience is not provided with an intervening activity to restore attitudinal freedom, a series of restrictive communications should meet with less success if they are

attributed to several sources. Rather than convincing the audience that the position taken in the message is widely shared, the tactic may lead to feelings of being hounded on every side. Greater perceived threat may result from the use of multiple communicators than from repeated attempts by the same communicator.

Verification of the implications of this experiment must await a careful conceptual replication. The most important design alteration required would be the incorporation of a pair of control conditions. Needed are a condition in which neither communication contains a threat and a No Restoration-No Second Communication condition in which the preference measure immediately follows the threat. The importance of a No Threat Control is obvious in retrospect. Without it one can only speculate about the effectiveness of the first threat in arousing reactance. The value of the additional No Second Communication Control deserves a bit more discussion. The No Restoration manipulation was designed to prevent reassertion of freedom by focusing subjects on other issues. In the process, it may also have reduced reactance by interrupting the ongoing activity in the situation (Brehm, 1972, p. 3). A No Restoration condition in which the final preference measure is taken just after the first threat would allow an assessment of the attitudinal effects of both the Forced Restoration and the No Restoration manipulations. The inclusion of both proposed control conditions should serve to answer some of the questions surrounding the One vs. Two Threats differences

under No Restoration. How much of the effect is due to positive influence when the second communication contains no threat and how much to another arousal of reactance after repeated threats?

A replication would also benefit from procedural changes to avoid confounding familiarity with the issues and restoration of freedom. Ideally, the experiment should employ a different freedom than the attitudinal freedom of the Snyder and Wicklund (in press) paradigm. Attitudinal freedoms cannot unequivocally be manipulated in the way that behavioral options can be. Utilization of a behavioral options paradigm (e.g., Wicklund, 1970; Wicklund, Slattum, & Solomon, 1970) would not only aid in interpreting the results in terms of reactance theory, but also help to establish the generality of the Snyder and Wicklund (in press) prior exercise effect.

This experiment manipulated repeated threats to freedom. It is worth noting that the effect of repeated or successive eliminations of freedom may be altogether different. When a freedom has been unequivocally eliminated only an exercise of a related freedom or extreme distortion of the circumstances could count as a restoration of freedom. Thus, it is unlikely that any intervening activity on the part of an individual would attenuate the reactance aroused by repeated eliminations by weakening their credibility. Snyder and Wicklund (in press) have made a similar argument in regard to the effect of prior exercise on a subsequent elimination of freedom.

On the other hand, regardless of the intervening activity, repeated eliminations of the same or similar freedoms may eventually lead to less and less reactance. Repeated eliminations may redefine the situation for the individual as one in which he has no freedom. Without a perception of behavioral freedom no reactance should be aroused (Hammock & Brehm, 1966; Worchel, 1974). The implication of Wortman and Brehm's (1975) integration of reactance and learned helplessness theories is that the function representing magnitude of reactance across successive eliminations of freedom should be non-monotonic. Initially, repeated eliminations are predicted to increase reactance, but later in the series to reduce it.

In sum, the results of this first attempt to investigate the effect of successive threats to the same freedom suggest that they do, indeed, lead to further reactance arousal when no intervening reassertion of the freedom is allowed. Future research is necessary to provide a stronger demonstration of repeated threat effects and to extend the concept to behavioral freedoms. Pinpointing the psychological process underlying the increases in reactance and clarifying the role of the source of the threats will require the incorporation of additional variables and design refinements not included in the present study.

APPENDIX A

DESCRIPTION

Currently there is a position open in the North Carolina state government for chairman of the Governor's Committee on Jobs for Young People. This committee will have responsibility during the next few months to study the issues concerning the availability and importance of part-time and summer employment for students and graduates of North Carolina schools. When they have finished their study, the committee will report to the governor concerning whether the state should attempt to create more jobs for students, or whether the money could be better spent in other areas.

The top candidate for the chairmanship of the Committee on Jobs is James Tyson. Here is a brief description of Mr. Tyson:

James Tyson

Born in Charlotte, N. C. in 1935.

Resident of Durham.

Graduated from the University of North Carolina as a business and government major. Later earned a master's degree in business administration from U. N. C.

Has operated his own office equipment and supplies business for most of his career.

Belongs to the Methodist Church, the North Carolina Businessmen's Association, and the Junior Chamber of Commerce of Durham. Spent one year in Washington, D. C., as a board member of the American Council of Small Businesses.

When asked about the need for more part-time and summer jobs for students in North Carolina on previous occasions, Mr. Tyson has expressed a willingness to consider a wide range of proposals and programs. Mr. Tyson also has a good relationship with young people and is often invited to talk to student groups and clubs.

APPENDIX B

FIRST COMMUNICATION

Franklin Bradshaw, an administrator at North Carolina State University, has commented on the merits of James Tyson for the post of chairman of the Governor's Committee on Jobs for Young People:

There are a number of reasons why you must conclude that Tyson is the only rational choice. First of all, his location in Durham puts him close to a large number of students seeking part-time and summer work. Close contact with the people who will need short-term employment in Durham, Chapel Hill, and Raleigh can be important in conducting an adequate study of the situation.

Secondly, his background in business organizations puts him in touch with the individuals who do the hiring. James Tyson is sensitive to the interests and desires of both management and labor.

Tyson's earlier statement that he is willing to consider different proposals concerning short-term jobs indicates that he is flexible on the issue.

There is no question that James Tyson is the man for the job. You have no choice but to agree with me.

APPENDIX C

FORCED RESTORATION ESSAY INSTRUCTIONS

We have found that one useful way to gather information about how people form their opinions is to have them argue strongly for one side or the other of an issue. Based upon what you now know, please write an essay in the space below arguing strongly against making James Tyson chairman of the Committee on Jobs for Young People.

APPENDIX D

NO RESTORATION ESSAY INSTRUCTIONS

We have found that one useful way to gather information about how people form their opinions on an issue is find out how they feel about related issues. In addition to the Committee on Jobs for Young People, it has been suggested that the Governor of North Carolina might establish committees to investigate new sources of energy, the prison system, and drug abuse. In the space below, please describe any committees you would like to see established by the Governor of North Carolina and explain why.

APPENDIX E

SECOND COMMUNICATION WITHOUT THREAT (ONE THREAT CONDITION)

(As I said before) I think Tyson would be a good man for the chairmanship. He has lived in North Carolina all his life, even his college degrees were from a North Carolina university. This background has provided James Tyson with many opportunities to visit with the people of the state and to examine their employment problems.

Furthermore, he has experience in running a business himself. Tyson is acquainted with the hiring practices, both long-term and short-term, that are necessary for a successful business.

Finally, Tyson spent a year dealing with national business problems. He should be familiar with ideas for putting an end to the summer job crunch from across the country.

(As I said in my previous statement) James Tyson would be a good choice for the position.

APPENDIX F

SECOND COMMUNICATION WITH THREAT (TWO THREATS CONDITION)

(As I said before) you must agree with me that Tyson is the only man for the chairmanship. He has lived in North Carolina all his life, even his college degrees were from a North Carolina university. This background has provided James Tyson with many opportunities to visit with the people of the state and to examine their employment problems.

Furthermore, he has experience in running a business himself. Tyson is acquainted with the hiring practices, both long-term and short-term, that are necessary for a successful business.

Finally, Tyson spent a year dealing with national business problems. He should be familiar with ideas for putting an end to the summer job crunch from across the country.

(As I said in my previous statement) James Tyson is the only reasonable choice for the position. You cannot disagree with me.

APPENDIX G

INTRODUCTION TO SECOND COMMUNICATION: SAME SOURCE

On another occasion, Mr. Bradshaw of the N. C. State administration had this to say about the value of James Tyson as chairman of the Committee on Jobs for Young People:

APPENDIX H

INTRODUCTION TO SECOND COMMUNICATION: DIFFERENT SOURCE

Jeffrey Rigsbee, a member of the North Carolina House of Representatives, had this to say about the value of James Tyson as chairman of the Committee on Jobs for Young People:

APPENDIX I

FINAL QUESTIONNAIRE

Circle the number that best agrees with your feelings. Questions 1, 2, 3, and 4 are about James Tyson.

1. How much are you in favor of, or opposed to, making James Tyson the chairman of the Committee on Jobs?

-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Very much					Don't					Very much
opposed					care					in favor

2. How much do you like James Tyson?

-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Dislike very					Neutral					Like very
much										much

3. How honest do you think James Tyson is?

0	1	2	3	4	5	6	7	8	9	10
Not at all										Extremely
honest										honest

4. How intelligent do you think James Tyson is?

0	1	2	3	4	5	6	7	8	9	10
Not at all										Extremely
intelligent										intelligent

Questions 5, 6, 7, and 8 are concerned with the statement of Mr. Bradshaw, the administrator at N. C. State.

5. How qualified is Mr. Bradshaw to comment about who should be the chairman for the Committee on Jobs?

0	1	2	3	4	5	6	7	8	9	10
Not at all									Extremely	
qualified									qualified	

6. How valuable were Mr. Bradshaw's comments in helping you to form an opinion of James Tyson?

0	1	2	3	4	5	6	7	8	9	10
Not at all									Extremely	
valuable									valuable	

7. How much do you think Mr. Bradshaw wants to influence you about James Tyson?

0	1	2	3	4	5	6	7	8	9	10
Does not									Wants very	
want to									much to	
influence me									influence me	

8. How biased is Mr. Bradshaw?

0	1	2	3	4	5	6	7	8	9	10
Not at all									Extremely	
biased									biased	

9. How much do you care about who is chosen as chairman of the Committee on Jobs?

0	1	2	3	4	5	6	7	8	9	10
Not at all									Very much	

10. How free did you feel to decide that James Tyson was not the man for chairman?

0	1	2	3	4	5	6	7	8	9	10
Not at all									Completely	
free									free	

11. How much did you feel like disagreeing with the statement that was made about James Tyson's good points?

0	1	2	3	4	5	6	7	8	9	10
									..	
Not at all									Very much	

12. Which topic did you write about earlier in the study? (Check one.)

_____ I wrote against making James Tyson the chairman of the Committee on Jobs.

_____ I wrote about what other committees the Governor should form.

13. What is your job situation right now? (Check the ones that apply to you.)

I now have a job.

I am looking for a job or will be looking for one soon.

I am not interested in a job at this time.

14. Your year in high school is: (Check one.)

Senior

Junior

Sophomore

Freshman

15. Your age is: _____

16. Your sex is: (Check one.)

male

female

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BIOGRAPHY

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